therefore such as to suit the practical pharmacist rather than the analytical chemist. The substances are described under their several Latin synonyms, in addition to the French, German, English, and Spanish names. We find that each preparation is described as to physical and chemical properties, and then follows a very elaborate examination for the presence of impurities, in addition to methods of quantitative determination of the principal constituents. It should be a very useful addition to the pharmaceutical laboratory.

Mr. Scott-White's volume is one of the usual little books of chemical analysis tables. There seems to be nothing very remarkable about it, excepting the variety of types in which the formulæ are printed. The book, which is intended as a text-book for the various examinations of the University of London, Oxford and Cambridge Senior Locals, and the Kensington examinations, seems well adapted for its purpose. It contains a table of solubilities of common inorganic salts, which is a thing students rarely make use of; and an appendix of requirements in examinations, detailing apparatus, chemicals, &c., necessary for most of the elementary examinations in chemistry.

With all our science classes and the very general spread of scientific education throughout the country, it is still a sad fact that the great mass of the public and even of the middle-class educated public are wofully ignorant on common things. Even now it is somewhat out of place to talk in a drawing-room about oxygen: as to the mention of phosphorus or selenium, or metals like platinum or iridium, it is still more out of place. A great deal of this ignorance—ignorance possibly occasioned by dread—is doubtless caused by the very scientific science books that are in common use. We are almost entirely without books on general science that are sufficiently simple, and at the same time accurate, to convey a general but correct notion of ordinary substances, or to interest the ordinary reader in all these things around us. Why should not the properties of oxygen or phosphorus be quite as interesting reading as some of the three-volume novels? Mr. Lloyd Morgan in his very small book has evidently intended to supply to some extent this want by describing—not in simplest language, it might have been simpler—a few very common chemical and physical facts. It does not appear exactly from the preface for what class of readers it is intended, but it can scarcely fail to be useful if not interesting to any lay readers. It commences with the chemistry of a candle flame, and in that way passes on to the similar actions taking place in animals and plants, where of course carbonic acid comes into play, and we are led through carbonic acid to wood, coal, and diamonds, to the atmosphere, where the physical part comes in, the pressure of the atmosphere, the thermometer, and the idea of elements, compounds, and mixtures. Passing on to water, we have the proof of the composition of water, physical properties of water, which leads directly on to the phenomena of heat. Although only consisting of about 150 small pages, we are led up at the end to some chemical reactions, and an appendix on molecules. The whole book is arranged for experimental purposes, although the methods of performing some of the experiments are not given. It has been probably assumed that the experimenter should have conveniences supplied. The appendix on arithmetical questions seems scarcely required in such a work, but, excepting this, it is certainly a step in the right direction to bring a knowledge of common things into a simple and understandable shape.

"L. M. C.'s" text-book is a sort of chemical, physiological, and biological book, and is divided under the following heads:—Food, its composition and nutritive value; its functions; and its preparation and treatment. It is evidently got up for the purpose of preparing for the examinations, as it says in the introduction that a grant of 4*s.* will be given for a pass, and that payment upon the results of examinations of school children are made to managers. In spite of this a considerable amount of useful information may be obtained from it, although that information is not conveyed in the best possible style. The descriptions of substances like bread, for instance, are not by any means exhaustive. Under animal food or flesh, it is stated that "animal food is composed of the same materials as vegetable; it is formed of the same elements and presents the same approximate principles, and contains water and mineral matters of the same kind as plants." This is not very instructive. The section on food and its selection will be useful, but the main object of the book is evidently to prepare for the examinations on this subject.

If the spread of chemical teaching may be measured by the number of small books on qualitative analysis, it certainly has a great number of disciples. There is nothing very extraordinary in Mr. Stoddard's book, unless it be the importance that is given to the atomicity marks attached to the signs of the elements. We find that iron is described as Fe^{ii} and also Fe^{iiii}, whilst chromium is only put down as Cr^{iiii}. Nickel and cobalt are both marked as dyad and tetrad. Only the so-called ordinary elements and acids are treated of.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Reply to Mr. Grubb's Criticisms on the Equatorial Coudé of the Paris Observatory

In continuation of my first letter I now wish, in my turn, to criticise Mr. Grubb's instrument, and to show that in all respects it bristles with inconveniences. I discuss it as it is presented and explained by Mr. Grubb, and I wish to examine it successively—

(1) From the optical point of view.
(2) From the point of view of the mechanism employed.
(3) From the point of view of its application to astronomy.

(1) The optical point of view. The system adopted by Mr. Grubb is much inferior to that generally used in ordinary equatorials. The dialytic telescope only gives images free from colour for the point which lies in the axis of the object-glass. For all other points images present themselves under the form of spectra which are longer as they are further from the axis. This arrangement necessitates that the three lenses must be very exactly centred, which can be done with the ordinary achromatic object-glass where the two lenses are in contact in the same cell. It is with very great difficulty that this can be done in a dialytic telescope. While admitting, however, that it can have an optical axis common for all the lenses of which it is composed, this centering becomes very difficult when the images are broken by a plane mirror, the

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angle of which varies, and it becomes almost impossible in a broken equatorial, in which the eyepiece is independent of the moving part of the instrument, as proposed by Mr. Grubb. In a word, taking no account of the new and very grave causes of variability introduced by Mr. Grubb, this optical system is so unstable that its employment has been rejected unanimously by all astronomers and opticians. The least derangement of the position in the central mirror spoils everything.

The mobility of the plane mirror presents equally, from the optical point of view, a slight inconvenience. The quantity of light varies with the different angles of inclination, which renders the exact researches of the photometric very difficult. Without making one feel all its gravity, Mr. Grubb has, in truth, indicated the defect of this optical system. But in order to turn the difficulty he suggests that, since the field of view becomes smaller as the instruments become larger, we may content ourselves with observing at a central point. But this is an affirmation pure and simple. It is necessary in many measures of precision to have a large field of view. The contrary will present several serious objections. We have, in fact, to observe stars in relation with other stars, to measure, for instance, the difference of declination between a planet and a star of comparison. But we cannot make both these observations at the centre. The same thing will always be happening, in the case of comets, nebula, and clusters. It seems to me, on the contrary, that a telescope is more perfect the larger the field of view. Feeling thus, I have had made by Prazmowski, for my new equatorial *coudé*, achromatic eyepieces giving a very large field. For the observation of comets I have such an eyepiece, which magnifies fifty times and has a field of view such that I can observe a degree. For a telescope of twenty-seven inches we might have such an eyepiece with a field of twenty-four minutes.

From all which precedes, I think everybody will agree that the system proposed by Mr. Grubb is far inferior to that now employed in ordinary equatorials.

(2) The mechanical point of view. The instrument consists actually of an ordinary equatorial, in which the part which carries the eyepiece is replaced by a counterpoise. It presents therefore, from the point of view of stability, all the defects of the ordinary instrument. Additional causes of instability inherent to the design are—

The micrometer and eyepiece are completely independent of the principal mass, which necessarily gives rise to different defects of decentering between the separated parts. Moreover, in this instrument there are three distinct movements. In addition to declination and right ascension, there is a third, which consists of a differential movement round the axis of the mirror. This last one constitutes alone, from the point of view of stability, a complication which does not exist in the ordinary instruments. All the movements of transmission are broken at a right angle, and are four times as long as those of an equatorial *coudé* of the same size. There will therefore be such loss in transmission, one would never have the instrument perfectly adjusted and oriented. We see, from the mechanical point of view, there is such an incoherence between the different parts of the apparatus, that it is inferior to those now employed. And it really cannot be compared with mine, which is almost as stable as a transit instrument, and in which the movements of transmission are excessively simple.

(3) If we examine Mr. Grubb's instrument from an astronomical point of view, we see it is based on a principle which no astronomer can admit, namely, that it is superfluous to observe the greater part of the northern heavens. In many investigations among the most elevated in the astronomy of precision —let us take stellar parallax as an example—one is obliged to combine observations made at different epochs of the year, and it is only by the combination of measures thus obtained that the desired result is arrived at. These stars must be observed, therefore, in the northern part of the heavens as well as in the others, for the vicissitudes of climate do not permit the astronomer to observe exactly how and when he wants. The same necessity presents itself in the study of the double stars; to ascertain and to discard the systematical errors in the angles of position the astronomer is obliged to observe these stars in all the celestial regions. If one wishes to limit one's self to the exploration of one side of the heavens, one would lose precious opportunities and gratuitously introduce serious difficulties. There are also many cases in which this choice is not possible. Thus, if we wish to discover new comets every part of the heavens must be explored, and if one wishes to observe them they must be observed where they are.

Finally, permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole. This region of space, I take it, would be entirely closed to the observer with Mr. Grubb's arrangement. Any research, therefore, which touched the stars covering this large area could not be undertaken.

The independence of the micrometer of the rest of the instrument renders impossible any measures of precision. The orientation of the micrometer, in fact, is the fundamental base of every measure, and to do this preliminary work properly three or four successive operations have to be performed, and take the mean of the readings and adjust the apparatus by means of the circle of position. But this fundamental operation cannot be performed on Mr. Grubb's instrument. In fact, in practice, if one wished to take an angular measurement with this instrument, one would have to proceed somewhat in this wise: First of all it would be necessary to content one's self with one approximation as to the orientation; then to repeat this after every individual measure; and lastly to take into account the disorientation of the micrometer, to submit the readings of the circle of position obtained to fastidious computations with a view to compensate them. This gives an idea of all the inextricable complications in which one would find one's self involved in this case. In fact, to secure a simple observation of a comet it would be necessary to increase the readings and the calculation by four times, and after all one would only get a result inferior to that furnished by an ordinary equatorial. I don't believe there is a single astronomer in the wide world who would undertake observations of precision under such conditions.

It is quite true, as Mr. Grubb indicates, that the *oculaire* might be connected with the rest of the instrument, but then, new inconveniences of another order would arise. These, however, I will not discuss now, for, as I said at the beginning, Mr. Grubb's actual proposal is now alone in question. However this may be, I consider the conception of this equatorial is so defective, taken as a whole, that I do not think its adoption would be seriously recommended. Nor do I think that the project will go beyond its present stage, unless essential modifications are introduced, and in this case the instrument would become like my own.

Paris Observatory M. LŒWY

Dust-Free Spaces

I VENTURE to call attention to some points in connection with the observations on "dustless spaces," &c., as detailed in the report of Dr. Lodge's lecture published in NATURE, vol. xxix. p. 610.

Certain observations and studies of my own lead me to think that, if attention be given to the points to which I wish to call the notice of physicists, results of the highest importance may be reached by means of the method of experimenting developed by Dr. Lodge and Mr. Clark, and described in the report referred to.

Dr. Lodge's statement (p. 611) that "cloud spherules are falling, but falling very slowly," is true when these spherules are not at a higher temperature than the atmosphere in their neighbourhood. When, however, very small particles floating in the air become heated, they warm the air immediately surrounding them, and then these particles are either buoyed up by a small envelope of heated and dilated air clinging to their surfaces, or they are borne aloft by the local currents which they create by contact with the surrounding atmosphere.

Observations continued for nearly fourteen years have convinced me that in ordinary clouds these two methods of lifting are combined—that to a certain extent each of the spherules or very many of the spherules of clouds are buoyed by adherent heated and dilated air, and that the whole of the cloud, in many cases at least, becomes warmer than its neighbourhood in general, which adds to its buoyancy as a mass of intermingled air, water, and vapour.

These remarks apply also to small particles of matter other than water. The action is the same except in degree. The very high specific heat of water enables it to heat surrounding air more readily and quickly than other substances do, and as a consequence masses of water as in clouds are lifted more quickly and to a greater height than masses of other bodies having the same proportion of surface to weight.

If it be remembered that radiant heat passes uninterruptedly through air, *i.e.* that air is diathermous, it will be seen that radiations from a distance striking upon particles of athermanous bodies suspended in the air will cause these latter to heat the

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